Commencement at Harvard last year was enlivened by the vigorous speech of Charles Francis Adams, initiating what may almost be called a national discussion of the Greek question. This year the subject of 'academic degrees' is brought into prominence by a paper, published in the July *Century*, from the pen of Dr. Woolsey. It will not surprise us if a discussion of this subject, begun by one who has held with honor the post of president of Yale college, and is still a member of the degree-giving board, should run for the next twelve months, and draw out opinions as diverse as those lately printed on the comparative value of classical and scientific studies. Most of Dr. Woolsey's article is historical, with incidental references to his own opinions. Toward the close, however, he makes some suggestions with respect to the bestowal of honorary degrees which are worth consideration. He is heartily opposed to the random methods now in vogue of complimenting men who are accidentally brought forward. He does not object to the guarded admission of meritorious students to the lower academic degrees *causâ honoris*, when they have been prevented by illness or poverty from attaining their diplomas in a regular way; and in cases of rare and distinguished merit he would admit to the same honors "discoverers of important principles in science, who had had, perhaps, no public education whatever."

But in respect to what are now bestowed as honorary titles (the degrees of LL.D. and D.D.), he would allow any graduate to prepare, by the study of years, for the highest degree within his reach, whether he resides within the college or not. The proficiency of each candidate should be tested by rigid examinations. Thus a student of law or theology might first take a baccalaureate degree in either of these faculties, — say, four years after taking his B.A. degree, — and eight years still later he might offer himself as a candidate for the degree of doctor of laws or theology. As a protection against the confounding of titles honorably won with those bestowed by careless or feeble institutions, Dr. Woolsey suggests that the indication of a degree shall be followed by the name of the place where it was won. We imagine that it will amuse some readers, and amaze some others, when they read the melancholy statement, made by one who for nearly forty years has been annually creating honorary doctors, that "these honorary degrees are bestowed on no evidence of thorough learning in theology or in law, and thus are in no way certificates of deserving the honors, saving, that, for some reason or other, the corporation of a college regards the person thus honored as a man worthy of notice beyond most of his fellows."

About two months ago we urged the Massachusetts legislature to be slow in rejecting the offer of the U. S. geological survey to prepare at divided cost a topographical map of the state. We are glad to state that the committee on expenditures, in whose hands the matter was placed, reported favorably; both houses passed the resolve submitted; and the governor has now made the excellent choice, as commissioners, of Pres. Francis A. Walker of the Massachusetts institute of technology, Mr. Henry L. Whiting of Tisbury, and Prof. N. S. Shaler of Harvard college. The resolve appropriates forty thousand dollars, to be extended over at least three years. The names of the commissioners are a guaranty that the interests of the state will be well administered, and that the suggestions made in our columns will not be lost sight of.

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

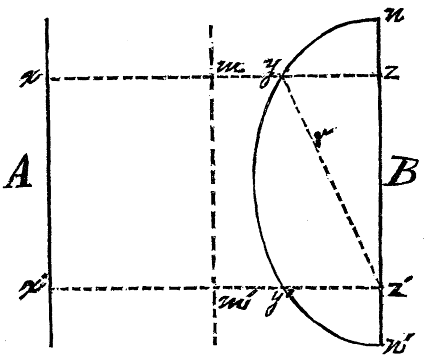
Radiant heat.

It is much to be regretted that a mathematical physicist of the standing of Mr. Fitzgerald should, in his letter published in your issue of May 16, confine himself to *ex cathedra* deliverances upon the question at issue between us, instead of attempting some direct demonstration upon the points involved, as I had suggested would be desirable. Had he done so, he would not, I am sure, have fallen into the curious mistakes which he emphasizes so strongly. In default of the desired investigation of the question by Mr. Fitzgerald, I hope that the following reasoning

may be of use in arriving at correct conclusions regarding this matter.

Let z and z' be taken as the foci of a semi-ellipse, $nyy'n'$, whose major axis is nn'; and let the eccentricity be so small that zy is greater than $\frac{1}{4}nn'$. Make $xz = nn' = 2xm$. Let a concave reflecting surface be supposed to be generated by revolving the semi-ellipse through angles of $+\frac{1}{2}\pi$ and $-\frac{1}{2}\pi$ about nn'; and let nn' represent a screen in which there are equal small circular apertures at z and z'; let there be also equal apertures at x and x'; and, in addition, let there be apertures at y and y' no larger than will permit the passage of cylindrical beams from the apertures at z and z' respectively.

At first let the apertures x and z' alone be open, and remain so until the spherical front of the wave-surface radiating from x has reached m, and a second wave-front of equal radius, $z'r$, has issued from z'. A part of this latter wave has, at the conclusion of this interval, been reflected from the concave mirror towards the focus z. Let the apertures at x and z' be then closed.



Next let the aperture at y be opened at the instant when the beams along xy and $z'y$ reach y, and be closed as soon as they have passed through y. They will pass through simultaneously, since $xy = z'y$.

Further, let the apertures z and x' be opened when the beam along xy reaches z, and let them be closed as soon as it has passed through z. The rays radiated from z', which were reflected from the concave mirror, will be brought to a focus at z, and pass through that aperture simultaneously with the beam in the direction xy; for, by the properties of the ellipse, the total distance traversed by any such ray is equal to $nn' = xz$: hence the wave-fronts, starting from x and z' at the same instant, will reach z simultaneously.

We have now to consider what occurs at each of the apertures y and z during the interval while they are open.

While y is open, a beam from x, of length xm, passes through it toward B, and a beam from z', of equal cross-section and length, passes through it away from B. These beams are of equal cross-section, because the tangent at y makes equal angles with the focal radii zy and $z'y$. But these beams are not of equal intensity in case A and B are of equal temperature, because any plane aperture, such as that at z', does not radiate equally in all directions. The intensity of the radiation diminishes, according to the well-known law, as the cosine of the angle between the direction of the ray and the normal; i.e., the intensity is less in the ratio of cos $yz'y'$ to unity: hence less heat has escaped at y than has passed through y toward B in the ratio just mentioned.

Now as to the quantities of heat passing through the aperture z. Let us for definiteness take the body B to be common air, enclosed in a capacious vessel whose interior walls are perfectly black. Such being the case, whatever be the intensity of the ray received through z in any given direction, the intensity of the ray simultaneously emitted through z will depend only upon the previous temperature of B, or, at most, only infinitesimally upon the intensity of the ray received. Such being the fact, the beam emitted from z in the direction of y' has the same intensity as that previously emitted from z' towards y. But the beam which is received at z by reflection from y' has a very different intensity from this, for it is the beam which was originally radiated from z' towards y'.

When, therefore, Mr. Fitzgerald says, that, "if heat can go into B in the direction $y'z$, there would be an escape of heat from B in the direction zy' as well as in the direction zy, and so, to the two quantities of heat coming into B, there would escape two equal quantities," I feel that either he has made a mistake, or he presumes upon the ignorance of the reader; and, to use his own inimitable emphasis, I may say that I am sure no American or other scientific man agrees with him; and I think I am justified in adding that no Irishman will agree with him either, including his own better self. To make this point still more evident, we have only to consider what occurs when the concave semi-ellipsoidal reflector without apertures at y and y' is used to transmit radiations alternately between z and z'. First let z be opened during an interval such that rays of a length $\frac{1}{2}nn'$ are emitted; then let both z and z' be closed for an equal interval; next let z' be opened for an equal interval. During this third interval, equal quantities of heat pass through z', towards and away from B; but is Mr. Fitzgerald now ready to re-affirm his untenable proposition that the quantities of heat received and lost in any arbitrary direction are equal? Whether he is willing to do so or not, these quantities are not in general equal, his hasty affirmation of their equality to the contrary notwithstanding.

In close connection with this, it is pertinent to inquire once more what difference there is between the equal quantities of energy which B has simultaneously emitted from and received through z'. The kind of energy we call heat exists in two forms, — radiant and non-radiant; the latter is often regarded as identical with molecular agitation. Radiant heat may be totally reflected regularly, as light is by a perfect reflector; it may be totally reflected irregularly, as light is at a white surface; it may be wholly absorbed, and the energy conducted or radiated away with a different wave-length, as light is at a black surface; it may be wholly transmitted, as light is by a transparent substance; or there may be any combination of these. It is sufficient for our purpose to suppose that the constitution of the body B is such that regular reflection does not occur at its surface, and that the absorption of the rays entering it takes place in its interior, as in a partially or completely transparent substance enclosed in a black vessel. Now, when the rays have been absorbed, as they must be under such circumstances before they can be radiated away from B, their energy exists in the non-radiant form. I have stated in my previous letter, that, "after the energy reaches B, the path by which it has arrived is of no consequence," and that the direction which the rays may have had in coming to B is immaterial to the question under discussion. I stand ready to re-affirm this proposition, and now do so. Mr. Fitzgerald evidently regards this statement as so unscientific as to merit no reply what-

ever, and as such a self-evident piece of stupidity as to render further discussion useless.

Mr. Fitzgerald further says that Professor Wood has pointed out my mistakes. Is he willing to say what mistakes? I am convinced that Mr. Fitzgerald has never read any criticism by Professor Wood which he is willing to indorse; but, since he has himself made reference to these criticisms, I now ask Mr. Fitzgerald to state which of Professor Wood's positions against me he regards as sound. I do not believe he can find one.

Mr. Fitzgerald is unable to find any excuse for me when I introduce the idea of a pencil of rays of infinitesimal angle, unless it be that I have overlooked the fact that the energy of such a pencil is infinitesimal. I beg leave to say that the excuse and the assumption are both entirely gratuitous on his part, and not in accordance with the facts. In the algebraic investigation made in the original paper, as well as in that given above, the angle is not assumed to be infinitesimal, or even small. The sole excuse, and the real one, was that it was a form of argument which it seemed to me would put in a clear light the truth which I had otherwise established, that such a process as had been proposed would heat B at the expense of A.

In conclusion I may be permitted to say, that when Mr. Fitzgerald attempts to treat the controversy which he has himself inaugurated as not worth his consideration, and gives notice that he therefore thinks it not worth while to continue it, he must know that he lays himself open to the suspicion that poverty of arguments, and not disinclination to controversy, leads him to this decision. If Mr. Fitzgerald regards it as compatible with his dignity to beat a retreat on any such pretext, I, for one, cannot agree with him. H. T. EDDY.

Cincinnati, June 10.

Temperature of the spheroidal state.

In some experiments made to determine this point, to avoid radiation, the temperature was measured by a thermo-electric couple, as in Mr. Hesehus's studies. The element used was composed of german-silver and iron, No. 22 wire. The wires were hard soldered together, and then bent into a loop, and inserted in a glass tube filled with plaster-of-Paris. The tube was about twelve centimetres long and five millimetres bore; and the polished loop projected about eight millimetres, with a width of four millimetres. This element was connected directly with a reflecting galvanometer with twenty-five ohms in circuit. The spheroids were formed in a spoon heated over a spirit-lamp, and no special precautions were taken to secure equal temperatures. The loop was plunged in the spheroid, and deflection noted. Ten readings were thus taken with very small variations, and then the loop was placed in a beaker of water almost in contact with the bulbs of two thermometers. The water was then heated till the deflection was the same as that given by the spheroid, and the thermometers were read at this point both while heating and cooling. The variations of temperature were less than 1°; and this part of the experiment was repeated several times. The whole experiment was repeated a number of times on different days, with results all within 1°.

The temperature thus found was, for water, 90°, and for alcohol, 69°.

The size of the spheroid had no effect on the temperature, as the deflection remained constant as long as there was enough liquid to protect the loop from radiation. In the case of alcohol, the globule could be surrounded with vapor-flames until greatly reduced in size, without visibly increasing the deflection. Ether was experimented on; but the temperature proved to be so low, barely above that of the room, that no satisfactory results could be obtained.

The series of experiments hints at a lower and less variable temperature than has usually been assigned to the spheroidal state. LOUIS BELL.

Dartmouth college, June 9.

The inventor of the vertical camera in photography.

In *Science*, No. 70, Mr. G. Brown Goode says, concerning the invention of the vertical camera, "As a matter of fact, the vertical camera now used for photographing natural-history specimens, etc., is the outcome of a suggestion made in December, 1869, by Professor Baird."

As this letter is written to put on record the history of the invention of the vertical camera, it is necessary, in justice to myself and other inventors of a vertical camera, to state that the notes concerning the history of the invention were omitted from my original article (*Science*, No. 62) at the suggestion of the editor. The facts concerning the invention and use of the vertical camera known to me at present are as follows:—

In 1863 J. Gerlach published 'Die photographie als hilfsmittel zu mikroskopischer forschung,' in which was figured and described a vertical camera. In 1866 Montessier, in 'La photographie appliquée aux recherches micrographique,' described and figured a very much improved vertical camera. Both of these are figured and described in Frey, 'The microscope and microscopical technology' (New York, 1872). In 1872 John C. Moss invented a swinging vertical camera, which was described and figured in the U. S. patent-office report, October, 1877, p. 961, plate page 279. This camera was also figured in the *Scientific American* (1877) and in *Leisure hours* (1879). In 1877 also appeared a description and figure of a vertical camera by Schaefer, in 'The microscope and histology,' p. 295. The above, together with the letter of Mr. Goode, the note concerning Dr. Dannadieu's camera, and the papers by myself, constitute, so far as I know, all the published notices of a vertical camera.

By the courtesy of the gentlemen named below, I am enabled to make important additions to the history of this subject. John C. Moss, president of the Moss engraving company, in a private letter, says, "I remember having used a camera in a vertical position in 1858 to copy daguerrotypes and tintypes. . . . I also used the same arrangement to photograph some shells and other small objects." Dr. Deecke says, "I have used the camera in a vertical position since 1873. The simple alterations on the camera were devised by myself, and executed in the shops of the asylum." Prof. E. Ramsey Wright, of Toronto university, also uses a vertical camera; but the date of its invention by him is not known to me. To briefly summarize: the first figure and description of a vertical camera known to me were those of Gerlach, in 1863; while the first to use the vertical camera was John C. Moss, in 1858. Every person using this instrument, so far as appears at present, was *an* originator, but John C. Moss, seems to have been *the* originator, of the idea of a vertical camera.

SIMON H. GAGE.

Ithaca, June 21.